

or months would probably eject as much or more dust and ashes than accompanied the Krakatoa convulsion, though not to so great a height. If, however, Mr. Preece's theory of electric repulsion of the dust particles be true, then the finest of them, if highly electrified, might rise to great heights, independent of the force of ejection from the volcano.

In this connection it is well to remember that there may have been many other volcanic outbursts during the last few months, of which we have not yet heard, and perhaps never may. The whole chain of islands from Java to Alaska, including the Philippines and Japan, is full of volcanoes, and seems to be a sensitive seam in the earth's crust. A convulsion like that of Krakatoa is likely to be accompanied or followed by others along this line, the northern portion of which is only visited by otter-hunters.

Without presuming to question the theory as to the rapid transmission of Krakatoa dust by the upper currents of the atmosphere until we see the evidence on which it rests, it occurred to me that the above considerations might possibly modify or supplement it in some degree.

Referring to the remarkable results deduced by General Strachey, showing an atmospheric wave travelling three times round the globe from the Krakatoa eruption, which seems to be of even more scientific interest from a physical point of view than the transmission of the dust and ashes, and which deserves a thorough and careful re-examination when the data are in from all available barometric records, I would say that I have been kindly allowed to examine the barometric records of the Signal Office here at Washington, and I find no trace of any such disturbance following the reported Alaskan eruptions of October 6 and October 20. In connection with the record of the waves following the Krakatoa catastrophe there are some interesting points which I wish to examine more carefully before discussing them.

H. M. PAUL

Washington, January 8

REFERRING to Mr. Burder's letter in NATURE of January 10 (p. 251), is it so certain that, if there be no resisting medium in interplanetary space, the whole of the earth's atmosphere must "rotate with the earth as if it were part and parcel of it"? Take a stratum of the atmosphere at, say, forty five miles in altitude at the equator. According to the received theory, this ought of course to move with a velocity greater than that of the surface of the earth immediately below. But each successive inferior stratum moves with less velocity. And thus they must tend to retard the superior strata with which they may be assumed to be in contact. Of course the merging of stratum into stratum is gradual, but this does not affect the amount of friction and retardation.

In like manner, imagine a section of the atmosphere taken along the equator. Sections taken along successive parallels of declination north and south would tend to retard the velocity of this central layer.

These two causes combined might have a considerable effect in retarding the velocity of the upper atmosphere in equatorial regions. And it seems to me doubtful whether the upper atmosphere near the poles would be actually carried round with each terrestrial rotation. The rarity of the upper regions of the atmosphere and the lessened force of gravity would both help towards the result indicated, inasmuch as they would tend to make the atmosphere less rigid.

As I am writing, I venture to make another suggestion. Gilbert White mentions that in the summer of 1783, when, as at present, the atmosphere was filled with dust consequent on volcanic eruptions, and "a peculiar haze or smoky fog prevailed for many weeks in this island and in every part of Europe, and even beyond its limits," "all the time the heat was so intense that butchers' meat could hardly be eaten the day after it was killed, and the flies swarmed so in the lanes and hedges that they rendered the horses half frantic, and riding irksome." May not the present May-like weather be due to a like cause? Sweet violets, primroses, wallflowers, roses, and several other flowers are now blooming in my garden under the Cleveland Hills.

Had the halos round the moon seen here last and the previous night any possible connection with the dust in the atmosphere? I computed the diameter of the inner dirty white to be twice, the dirty orange one and three-quarters, and the outer green three and a quarter times the moon's apparent diameter.

JOHN HAWELL

Ingleby Greenhow Vicarage, Yorks, January 15

I THINK a few notes relating to the recent sunsets may still have an interest for some readers of NATURE. Notwithstanding the length of time these remarkable phenomena have been apparent, the sunsets of January 11 and 12 were as brilliant as regards the *second* after-glow as any that have preceded them, the final glow having lasted on the 12th till 5.55; while the sun set that evening at 4.12.

The pink halo so often seen of late could not be discerned that day though the sky was cloudless; but it has been often visible when clouds partly obscured the sun, or portions of the sky, and could then be recognised between them, separating the blue of the remoter sky from the whitish light surrounding the sun, as a ring-formed glow of a strong pink colour.

These broad pink halos have been less commented on than the splendid sunsets which have invariably succeeded them, but they have been nearly as persistent in their presence. You have had so many accounts of the succession of colours and effects of the two after-glows, that I will not allude further to them here; but as I have retained a record of many remarkable sunsets and sunrises which I observed in Wales in former days (possibly the very same mentioned by Prof. Piazzi Smyth in NATURE, December 13, 1883, p. 149, as observed by him thirty years ago), and as I carefully noted in them the time and hour of the changes in the sky down to that of the complete extinction of the after light, it may interest others than myself to compare displays of that date with those of this winter.

What is worthy of especial interest is the great difference between the periods of prolongation then and now of the illumination of the western sky, showing that the second after-glow of recent sunsets is a phenomenon distinct from and additional to those belonging to normal sunsets.

The following table exhibits the two series of observations made in 1855, 1856, 1857, and in 1883-84 respectively:—

Date	Sunset at	First after-glow or cone of pink light		Sets	Second after-glow which begins as the first after-glow sets
		Appears as sunset colours are fading	Brightest at	1st series, 1855-56-57	2nd series, 1883-84
Nov. 11, 1856	4.13	4.50	4.57	5.10	5.45
" 12, " "	4.14	"	"	4.55	5.45
" 23, 1855	"	"	"	4.20	"
Dec. 7, " "	3.50	"	"	4.20	"
" 8, " "	"	"	"	4.20	"
" 10, " "	"	"	"	"	"
" 11, 1883	"	"	"	"	"
" 14, " "	"	4.15	4.21	4.35	"
" 15, 1856	"	"	4.20	4.30	"
" 18, " "	3.50	"	"	4.30	"
" 19, " "	3.53	"	"	"	"
" 24, 1883	4.13	"	"	"	"
Jan. 11, 1884	"	"	"	"	"
" 12, " "	"	"	"	"	"
" 15, 1857	"	"	"	"	"
" 16, " "	"	4.50	"	4.55	5.45
" 19, " "	"	"	"	5.0	5.40
" 26, " "	"	"	"	5.18	5.55
Feb. 8, " "	5	"	"	5.25	"

The colours associated with the actual sunset are quite in accord in both.

The first after-glow, or pink cone or dome of light appearing after the sunset colours have nearly faded, is also similar in both series, but its time of setting has been apparently somewhat prolonged in the recent observations.

It is the 1883-84 series alone, however, that shows the *second* after-glow, and the duration of this strange phenomenon, which I have the advantage of observing over a wide bird's-eye view in North Wiltshire, has extended on evenings when it could be well

observed to about one hour after the first after-glow had disappeared below the horizon. The exact moment of this disappearance has been more difficult to determine than in the earlier observations where darkness followed; as recently the heavens and the earth have been reilluminated just as the natural night would have begun.

T. STORY-MASKELYNE

Salthrop, January 13

As the "halo" exactly opposite the sun, reported by Mr. T. W. Backhouse in *NATURE*, January 10 (p. 251) may prove to be of considerable importance, I beg to add my observations of it on the 12th. I had noticed a mass of ruddy colour under the given conditions, previously, but had not detected its strange nature. The sunset on the 11th was very fine. The 12th, until after sunset, was cloudless, except for the hazy masses which seem to precede every sunrise, and, more especially, sunset, at present. Our sunshine record is an unbroken scorch from 9.15 a.m. to 2.52 p.m. (sun seen clear of horizon at 8.26, and touched at 4.0); I doubt if, previously, we have recorded even five hours in early January. At 7.45 a.m. on the 12th (sun rose at 8.22) the cloud-glow had turned to silvery green below, and rose from 15° to 30° in the south-east. At 7.47 the rose reached 60° , but was fainter. I first noticed the "halo" at 7.52. It was then so well defined that, calling a lady's attention by asking what she saw there, she spoke of it as "a broad rainbow." Position, by compass, 30° north of west. It was a semicircle situated 10° above the horizon, standing on the dark gray arch of dawn, Jupiter being on a line with the base of the left end of the rosy arch. The inner arc of this measured 10° , and the outer 24° in radius, but it spread out to 30° at the base. The centre was of the same blue as the sky to the right and left of the rosy semicircle, above the gray. The base, sinking faster than Jupiter, spread out so that, at 8 o'clock, the arch having now broken above, its outline was rather like a railway chair. The base now reached from west-north-west to north-north-west by north. After sunset there were signs of a similar phenomenon, but clouds prevented certainty.

Is not fifty miles an underestimate for the altitudes of the light-reflecting material? If Mr. Symons is nearer the mark in his suggestion (100 to 200 miles), then more than half of the eastward velocity of the original erupted dust is accounted for by retardation, due to matter having velocity belonging to an earth radius of 4000 miles, revolving in a circle of 4100 to 4200 miles radius. Would it need an eruptive force of more than two to four miles per second (six to twelve times greater than a cannon ball) to attain such altitudes? The constant uprush would minimise the air-resistance enormously.

York, January 14

J. EDMUND CLARK

P.S.—January 15.—This morning, at 7.47, the "halo" began to form, but was not nearly as perfect as on the 12th. The arch (upper part only) was *rayed*, as if it were the opposite point of sight for rays from the sun. All over before 8, or fully twenty minutes before sunrise.—J. E. C.

WITH reference to Herr Wetterhan's inquiry as to the absence of the sky-glow in a clear sky at other places than Freiburg on the morning of January 11, I find that at San Remo, in Northern Italy, where I spent the week ending on that day, a similar falling off of effect occurred at the same time. The sunrise was "very fine, but nothing to compare with the sunset of yesterday," and "the filmy streaks were very thin, and stretched this morning from south-west by south to north-east by north." Nevertheless there was the strange bluish-white glare above the eastern horizon, casting shadows, and a thin pink film up to about 75° at 28 min. before sunrise. The sunset glow of this day and of the day before was magnificent, the procession of colours beginning about 15 min. after sunset, and lasting a full hour. I see that your Constantinople correspondent also mentions the sunset of the 11th as a remarkably fine one. The air on the 10th, not the 11th, as at Freiburg, was wonderfully transparent at San Remo, the whole range of Corsican mountains, over eighty miles distant, standing out sharply for 15 min. before and after sunrise, and the sun himself bursting forth in great splendour from below the sea line.

London, January 19

F. A. R. RUSSELL

Unconscious Bias in Walking

SOME ten or twelve years ago I made some experiments upon the subject of Mr. Larden's letter in *NATURE* (Jan. 17, p. 262),

namely, unconscious bias in walking. The experiments were not numerous, but they left no doubt in my mind as to the cause of divergence from a straight path. My notes were sent, at my father's suggestion, to the late Mr. Douglas Spalding, who was about to undertake experiments on the curious power which animals have of finding their way. I rather think he made some trials with pigs, but I believe he never published anything on the subject. In stating my results I am compelled therefore to rely on memory only.

I began with walking myself, and getting various friends to walk, with eyes shut in a grass field. We all walked with amazing crookedness in paths which were not far removed from circles. I myself and Mr. Galton on the first trial described circles of not more than fifty yards in diameter, although we thought we were going straight, and afterwards I was generally unable to impose a sufficiently strong conscious bias in one direction to annul the unconscious bias in the other. I believe we all diverged to the right excepting one of us who was strongly left-handed.

I then got eight village schoolboys, from ten to twelve years of age, and offered a shilling to the boy who should walk straightest blindfold. Before the contest, however, I dusted some sawdust on the ground, and after making each of the boys walk over it, measured their strides from right to left and left to right. They were also made to hop, and the foot on which they hopped was noted; they were then made to jump over a stick, and the foot from which they sprang was entered; lastly, they were instructed to throw a stone, and the hand with which they threw was noted. Each of these tests was applied twice over.

I think they were all right-handed in throwing a stone, but I believe that two of them exhibited some mark of being partly left-handed. The six who were totally right-handed strode longer from left to right than from right to left, hopped on the left leg, and rose in jumping from that leg. One boy pursued the opposite course, and the last walked irregularly, but with no average difference between his strides. When told to hop, he hopped on one leg, and in the repetition on the other, and I could not clearly make up my mind which leg he used most in jumping. When I took them into the field, I made the boys successively take a good look at a stick at about forty yards distance, and then blindfolded them, and started them to walk, guiding them straight for the first three or four paces. The result was that the left-legged boys all diverged to the right, the right-legged boys diverged to the left, and the one who would not reveal himself won the prize. The trial was repeated a second time with closely similar results, although the prize-winner did not walk nearly so straight on a second trial.

I also measured the strides of myself and of some of my friends, and found the same connection between divergence and comparative length of stride. My own step from left to right is about a quarter of an inch longer than from right to left, and I am strongly right-handed.

Comment on these experiments seems needless, and they entirely confirm Mr. Larden in his view.

It seems to be generally held that right-leggedness is commoner than the reverse; this I maintain to be incorrect. I believe that nine out of ten strongly right-handed persons are left-legged. Every active effort with the right hand is almost necessarily accompanied by an effort with the left leg, and a right-handed man is almost compelled to use his left leg more than the other. I believe that Sir Charles Bell considered that men were generally right-legged, and sought to derive the custom of mounting a horse from the left side from the fact that the right leg is stronger than the other. I suggest as almost certain that we mount on that side because the long sword is necessarily worn on the left, and would get between our legs if we went to the off-side of the horse. Some of your readers may perhaps be able to tell us whether the Chinese do not wear their short swords on the right and mount their horses from the right.

I will not hazard a conjecture as to why the rule of the road in Great Britain, and inside of the towns of Florence and of Salzburg (?), is different from that adopted by the rest of the world. For an armed horseman the English rule is, I presume, more advantageous, both for attack and defence.

January 20

G. H. DARWIN

THE question whether a man will walk to the right or left in a mist, in darkness, or if blindfolded, has led to no little controversy and dispute. Almost every conceivable reason has had its advocates for the fact that some men persistently turn to the